TREASURY DEPARTMENT

UNITED STATES
PUBLIC HEALTH SERVICE

WASHINGTON, D. C.

OFFICE OF THE DIRECTOR
HYGIENIC LABORATORY
TWENTY-FIFTH AND E STREETS NW.
Address reply:
Cosmos Club

March 5, 1926

Dr. Florence R. Sabin
Rockefeller Institute
66th Street and Avenue A
New York City

Dear Doctor Sabin:

I have been tremendously interested in your last two letters. I had two interesting conferences with Dr. Cunningham and feel relieved about the whole situation. I expressed to him my desire, as Chairman, to work with you as cousellor in this field and he accepted very generously. That may now be considered, I think, accomplished. It will only be necessary that we keep each other fully informed, then, I think, no danger can arise.

I have not been able to do anything with the oxidase work in the last three weeks as I have had to finish up work on Sanocrysin and the Tuberculin Conference. Whenever possible however I have been informing myself through the references which you have sent and such other reading as I can do. I expect to be in New York at a Research Committee meeting and International Union meeting Thursday, Friday and Saturday of next week and will find sometime to come out and see you. I hope to try scraping rats, rabbits and guinea pigs lungs and use the Japanese oxidase reaction before I come but may not be able to do it.

I have gone over the McJunkin article twice and I think he misses the point. There seems to be no doubt from your work that there is a common stem cell. I am not sure however the problem of de-differentiation can be worked out on this group of mononuclear cells on the bases of their phagocytic attributes. They are phagocytic for such a wide range of colloids and the India ink method especially seems futile to me. The neutral red rosette seems a much more specific thing. These methods seem to me to miss the main point which is - that in this group of cells there is one with peculiar chemical properties which has a definite relation to the existance of the tubercle bacillus in the body at a certain period of its existance therein. It may be that the peroxidase granules are the key to it but this may not be true. That there is some chemical peculiarity in one group of this family of cells that makes it a factor in tuberculosis of all animals I feel surer and surer as your work progresses. It is with the idea of finding this peculiarity that we have thought that Chamber's assistance would be very valuable. If we can devise,

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through the micro-chemist, methods which will bring us the knowledge that is involved in your observations of the changes in the cell by one of the Johnson fractions and get nearer and nearer to the specific factor that makes this cell a peculiar chemical factory related in sympathy to one or other strain of tubercle bacillus, then I think, we shall be on our way, and you and Dr. Doan have laid the foundation.

There seem to me to be two phases that require immediate study - one is the peculiarity of the monocyte in different animals, - and, the other is the development of the peculiarities in the different strains of tubercle bacillus. There is no doubt that there is a great variation in the granules and spores which the acid fast family produces. I shall bring you up some slides demonstrating this from our recent efforts here to degrad some organisms to their earlier evolutionary types.

I have recently been reading the histological picture of Jöhnes disease and it is quite apparent that if the premise on which we have been working is correct there must be some chemistry peculiar to the monocyte family in the intestine ruminants which makes them susceptible in this region to another member of the family of acid fasts. I therefor want to talk over with you Cunningham's suggestion that he has been able to satisfy himself that there is a distinct difference between the monocyte of the rabbit and guinea pig. The Lewises have found differences in frogs. Pappenheim called attention to the difference in large mononuclear cells in different animals. The whole question seems to me to hinge on the development of finer and finer methods in the study of cell chemistry. Chamers seems to have the technique available and you have opened the door for such a development. The peroxidase reaction is in line with the general plan for the study of cell chemistry.

Hoping to see you next week and with the kindest regards, I am

Very sincerely yours,

W Charles White

dcw

William Charles White, M.D.